be recorded the carotid pulse, the respiration, the time in seconds and the rate of muscular movements. Experiments made with this apparatus show that the curve of carbon dioxide excretion during work closely resembles that of the pulse, and that carbon dioxide is at least in part the cause of the secondary rise in the pulse rate observed by Bowen.

Dr. W. B. Pillsbury detailed some experiments on 'The Attention Wave as a Measure of Fatigue.' Not merely the daily rhythm of fatigue and practise of the typical morning and evening workers was reflected in the ratios of the period of visibility to the period of invisibility in the attention wave, but the degree of fatigue on days of severe work as compared with easy days had a corresponding variation in the fluctuation of attention. In the morning, practise shows itself in a continuous increase in efficiency through at least a considerable portion of the experiment; while in the evening there is a decreasing effectiveness almost from the beginning. As further substantiation of the theory that the attention wave is closely related to the Traube-Hering or Mayer vaso-motor waves, it was noted that both have the same daily rhythm of length.

Frederick C. Newcombe,
Secretary.

Discussion and Correspondence.

Morgan on Evolution and Adaptation.

To the Editor of Science: I have always supposed that what are generally called Lamarckian views of evolution were considered with less prejudice by biologists in the United States than in England or Europe, and that my own publications in support of such views were, therefore, likely to be known and read in America even if they were almost completely ignored by my own countrymen.

I find, however, that Dr. Thomas Hunt Morgan in his book 'Evolution and Adaptation,' which has just appeared, makes no mention whatever of my book 'Sexual Dimorphism in the Animal Kingdom, a Theory of the Origin of Secondary Sexual Characters,' which was published in London more than three years ago. Any biologist, American or other, has a perfect right to reject all my conclusions, but it seems to me that an author who devotes a great part of his book to the discussion of Darwin's theory of sexual selection and the evolution of secondary sexual characters, in entire ignorance of the facts and arguments which it cost me years of labor to collect and elaborate, lays himself open to the charge of writing without proper knowledge of the literature of his subject. I have published the results of experimental work apart from this, but the only reference Dr. Morgan makes to it is to a popular article in *Natural Science;* he has not apparently consulted the original memoirs.

Like other English writers it has been my ambition that my work should be known to the scientific public of the United States, which is not only very intelligent but free from prejudices which are stronger than reason in England. I am much disappointed to find that my chief contribution to the investigation of evolution is so little known to American evolutionists.

J. T. Cunningham.

Zoological Society,
3 Hanover Square, London, W.

Mutation and Selection.

In reading Professor Morgan's very interesting and valuable book, 'Evolution and Adaptation,' it is surprising to find that he apparently regards the theory of evolution by selection and DeVries's mutation theory as being to a degree in conflict.

The evolution which observation shows us has taken place is chiefly characterized by the fact that it has brought organisms into favorable relation with their environmental conditions. That this could have been secured by mutation unaided by selection seems altogether unlikely.

In the case of the leaf butterflies of the genus *Kallima* the theory of evolution by mutation alone must assume that the remarkable resemblance arose all at once by a single mutation, or that there were a series of mutations which for some unaccountable reason were of such a character as to make the resemblance to a leaf gradually grow more perfect, though no selective action of the environment controlled this improvement in pattern.

The first assumption, of the origin of the perfect leaf pattern by a single mutation, is unsupported by evidence and to me seems very improbable. That the resemblance arose by the cumulation of a series of mutations independent of selection seems no less improbable, for in this case we have either to assume some mysterious internal regulation of the mutations directing them all in one direction, or else we must assume that among the many possible mutations only those that were in the direction of closer imitation happened to occur. The latter is of course practically impossible upon the theory of probabilities and the former leads us into a realm of darkness which we seem at present unable to explore. If, however, there is reason to believe in such internal directive influence, we are not justified in rejecting it because of our inability to study its nature and action. I can not see that we have such evidence.

I have been impressed with the feeling that Professor Morgan has allowed his opposition to Darwin's conception of evolution by the selection of favorable 'fluctuating variations' to cause him to understate the importance of selection, though in parts of his book he recognizes that selection acts on mutants and variants. The Darwinian theory and the theory of evolution by selection are not identical, yet Professor Morgan frequently refers to them as if they were so. If mutations be distinct from fluctuating variations, as our as yet very scanty evidence seems to suggest may be the case, still both mutations and variations, so far as we can see, would be subject to selection. The theory of selection is an explanation of some of the phenomena of adaptation. It is difficult to see that the mutation theory, apart from selection, aids us in understanding or imagining how this adaptation, the most general phenomenon in organisms, has been secured.

Mutation may be the mode of origin of certain useful qualities, but it is difficult to see how it explains their retention and perfection. The theory of selection makes no pretense to explain the origin of varieties or mutations. It attempts to explain the adaptation of organisms to their conditions of life, such adaptation resulting from the selection of those individuals which vary or mutate in useful directions. The theory of selection begins where the theory of mutation leaves off.

Not even a combination of DeVries's mutation theory with Weismann's theory of germinal selection would give us, without natural selection, an explanation of progressive perfection of adaptation. We should still need to add Nägeli's, or rather St. George Mivart's, perfecting principle.

The work of DeVries seems especially valuable since it brings to the front such questions as the following:

Are there mutations which are distinct from fluctuating variations? Are fluctuating variations restricted to rather narrow limits, and are the larger variations which occur of a different sort, establishing a new mean about which a new series of fluctuating variations cluster?

Are mutations (or variations) definite or indefinite? Do they follow certain lines or do they occur in all directions?

If the direction of mutations (or variations) is wholly or in part predetermined, what are these predetermining factors? Are they internal (involved in the nature of the organism), or external (environmental), or both?

Is there a tendency in mutants (or variants) to revert toward the condition of the parent stock?

Are mutants (or variants) of one sort more (or less) fertile or more (or less) vigorous when bred together than when bred with the parent stock or with mutants (or variants) of another sort? Does mutation (or variation) cause partial (or complete) segregation?

Are hybrids between mutants (or variants) of different sorts or between mutants (or variants) and the parent stock intermediate in character between the two parents, or do they follow wholly or chiefly one parent? If the latter, which parent is followed in the several kinds of crosses?

Upon most of these points the observations of DeVries have an important bearing, though, without much further observation, they do not decide them.

It seems possible that one of the most im-

portant results of the work carried on by and stimulated by DeVries will be to show another way in which partial segregation may be secured, and the theory of natural selection needs all the help it can get from segregation.

It should hardly be necessary to urge that, in understanding the development of the conditions which prevail to-day among organisms, the problem of the origin of species seems of very secondary importance in comparison with the problem of the perfection of adaptation.

Maynard M. Metcalf.
The Woman's College of Baltimore.

WILBUR WRIGHT'S SUCCESSFUL FLIGHT IN A MOTOR-DRIVEN AEROPLANE.

The newspapers of December 18 contained the announcement that Wilbur Wright had flown a distance of three miles with an aeroplane propelled by a 16-horse power, four-cylinder, gasoline motor, the whole weighing more than 700 pounds. To the average newspaper reader this meant no more than similar statements previously made in the newspapers that men had flown in New York, or St. Louis, or San Francisco. But to the student of aeronautics, and particularly to those who had followed the careful scientific experiments with aeroplanes which were being made by Orville and Wilbur Wright, it meant an epoch in the progress of invention and achievement, perhaps as great as that when Stevenson first drove a locomotive along a railroad.

It meant that after ages of endeavor man had at last been able to support himself in the air as does a bird and to land in safety at a spot chosen in advance.

The report from an authoritative source confirms the fact of this flight, but modifies the details somewhat from those given in the newspapers. It appears that four successful flights were made in a motor-driven aeroplane on December 17 near Kitty Hawk, N. C. The wind was blowing about 21 miles an hour and a speed relative to the wind of 31 miles an hour was attained by the aeroplane. This meant a speed of 10 miles an hour relative to the ground. The aeroplane had a surface of 510 square feet and in the longest flight was in the air 57 seconds. The aeroplane is said to have risen from a level. The reported distance of three miles was probably relative to the wind.

The earlier work of the Wright brothers is described in the reports of the Western Society of Engineers and in part republished in the Annual Report of the Smithsonian Institution for 1902. Their invention of a forward rudder has contributed to the final success.

The modern success in aeronautics may be said, I think, to date from the feat of Otto Lilienthal in 1891 in gliding down an incline in an aeroplane. These glides were repeated with much success and with an improvised aeroplane by Mr. Chanute and Mr. Herring in our own country. Mr. Herring even went so far as to carry with him 50 pounds of sand in his aeroplane which weight he computed would be that of an engine sufficient to support him.

Mr. Pilcher, in England, repeated these experiments on a level by rising into the air in his machine when drawn by a horse attached to a rope, the machine rising like a kite and then gliding forward. Mr. Whitehead is described in the *Scientific American* as having repeated this experiment recently in Connecticut with a motor on board the aeroplane.

In the meantime, in 1896, Dr. Langley had driven a model weighing about 25 pounds through the air with a small steam-engine, and Sir Hiram Maxim had performed the wonderful feat of lifting 7,000 pounds into the air for a moment. This was done with an aeroplane having 5,000 square feet of surface driven by serial screws attached to a steam-engine of 360 horse-power and of extraordinary lightness.

But, notwithstanding all these partial successes, there was, owing to the recently reported failure of Dr. Langley to lift a man and to other causes, a wide skepticism as to the possibility of human flight.

Mr. Wright's success in rising and landing safely with a motor-driven aeroplane is a crowning achievement showing the possibility of human flight. Much yet remains to be done, but with the stimulus of this beginning progress will probably be rapid. In the progress now achieved a great deal is due to Mr.